

address the story from a subaltern point of view is beset by two major challenges: the limits posed by the sources in establishing the experiences and motives of those who resisted Alexander, and whether the various forms of resistance to him can be seen as a useful overall category, or merely a hotchpotch that only makes sense from Alexander's point of view. Notwithstanding these challenges, Heckel has managed to produce a convincing answer that rests on three major premises. The first premise is that military resistance to Alexander was strongest in the periphery of the Persian Empire, in particular in Greece and the Indus lands. The second is that the best way to conceptualize Alexander's conquests is as a 'hostile takeover': for most people within the core areas of the Persian Empire there was little at stake apart from changing task-masters. The third premise is that the very success of the Persian Empire in its military and administrative structures was a major reason that a hostile takeover could succeed by taking advantage of them. This is undoubtedly a highly stimulating book, which will provoke significant reflections on many aspects of fourth-century BCE eastern Mediterranean history.

KOSTAS VLASSOPOULOS

University of Crete, Greece

vlasop@uoc.gr

doi:10.1017/S0017383520000303

This is an Open Access article, distributed under the terms of the Creative Commons Attribution licence (<https://creativecommons.org/licenses/by/4.0/>), which permits unrestricted re-use, distribution, and reproduction in any medium, provided the original work is properly cited.

Roman History

A bumper edition this time, by way of apology for COVID-necessitated absenteeism in the autumn issue. The focus is on three pillars of social history – the economy (stupid), law, and religion. First up is Saskia Roselaar's second monograph, *Italy's Economic Revolution*.¹ Roselaar sets out to trace the contribution made by economics to Italy's integration in the Roman Republic, focusing on the period after the 'conquest' of Italy (post 268 BCE). Doing so necessitates two distinct steps: assessing, first, how economic contacts developed in this period, and second, whether and to what extent those contacts furthered the wider unification of Italy under Roman hegemony. Roselaar is influenced by New Institutional Economics (hereafter NIE), now ubiquitous in studies of the ancient economy. Her title may be an homage to Philip Kay's *Rome's Economic Revolution*, but the book itself is a challenge to that work, which in Roselaar's view neglects almost entirely the agency of the Italians in the period's economic transformation.² For Roselaar, the Italians were as much the drivers of change as the Romans; indeed, it is this repeated conviction that unifies her chapters.³

After introductory matters, Chapter 2 traces the increase in actual contacts between Romans and Italians, both directly economic – in colonies (both Roman and Latin),

¹ *Italy's Economic Revolution. Integration and Economy in Republican Italy*. By Saskia T. Roselaar. Oxford and New York, Oxford University Press, 2019. Pp. xi + 297, 8 b/w figures, 2 maps. Hardback £70, ISBN: 978-0-19-882944-7.

² P. Kay, *Rome's Economic Revolution* (Oxford and New York, 2014).

³ Cf. here N. Terrenato, *The Early Roman Expansion into Italy. Elite Negotiation and Family Agendas* (Cambridge & New York, 2019), reviewed in *G&R* 67.1 (2020), 96–7.

migration, temporary work, pasturing, religious hubs, markets – and indirectly so – military service, marriage, friendships, patronage, and so forth. Roselaar suggests that, while plenty of scholarship has connected conquest and cultural change, the face-to-face connections by which this occurred have been assumed rather than interrogated. Opportunities for such contact were manifold, but many preceded rather than followed the ‘conquest’. In addition, Italians participated by choice, and in fact the evidence suggests that Italians saw themselves as equals to the Romans, rather than their subordinates, in most of these interactions. Chapter 3 considers the economic impact of these connections. Roselaar challenges the traditional model of the negative economic impact of the ‘conquest’ (e.g. by Roman confiscation of *ager publicus*, as evocatively suggested by Appian, *BC* 1.7–8). Instead, she sees in the archaeological data evidence for Italian economic prosperity in agriculture, pastoralism, manufacture, trade, and banking. In large part, Rome left the Italians alone in economic terms, but the changing landscape (in particular, the emergence of new markets) provided new opportunities from which Italians benefited. This economic impact was thus highly regional, as Roselaar demonstrates in four geographical case studies: Latium, Campania, Apulia, and Lucania/Bruttium. In general, though, the result was greater integration of Italy in the Mediterranean economy. Chapter 4 turns to long-term consequences, and the changes to formal legal and trading mechanisms that were catalysed by increased economic contacts and their impact. A discussion of the resulting Italian investment in the peninsula forms a segue to discussion of linguistic and cultural change, and thus to Italian ‘identity’, broadly understood, as it emerged in the third and second centuries. Roselaar’s argument is that the changes to the latter were prompted not by Rome itself, but by the Italian economic endeavours that Rome’s conquest made possible. That in turn increasingly produced an Italian–Roman binary, and Chapter 5 treats the process by which that binary became a divide. Roselaar traces the growth in Italian dissatisfaction in the second century, as the lack of citizenship impacted their economic horizons (by, for example, preventing them being *publicani*). She then turns to the economic and cultural impacts of the grant of citizenship that ended the Social War. In particular, though further economic options were now available to Italians, neither they nor the Romans viewed the grant as representing full cultural integration; for that, Augustus was needed.

Roselaar’s book is by no means an easy read; its methodical approach to documentation makes this something of a slog. Not printed in the book but available online is an appendix, in the form of an interactive map, containing the locations of nearly 10,000 named Italians of the period.⁴ Roselaar also incorporates the work of a vast array of other scholars (the bibliography contains around 650 items). Such detail makes it easy to lose one’s grip on the argumentative thread. But that labour is also the book’s strength, because the picture that gradually emerges is undeniably securely grounded. Roselaar shows that, while the Roman ‘conquest’ changed the economic landscape, Italians took advantage of that in diverse, regionally dependent ways. Their association with Rome was driven more by these economic opportunities than the cultural ones on offer. Moreover, cultural assimilation was not the cost of economic success; in fact, many Italians in practice remained unaffected by Rome in terms of identity, even as they exploited its infrastructure. Economic and cultural

⁴ <<https://global.oup.com/booksites/content/9780198829447/>>, last accessed 31 October 2020.

'Romanization' did not, in other words, go hand in hand. That adds important nuance to our understanding not just of the Republican economy, but of the Roman imperial project more broadly. This book thus takes its place as one more small but important piece of the new jigsaw we are collectively putting together of the Mediterranean's evolution – one which does justice to the experience and agency of all its inhabitants.

A similar drive towards economic synthesis, but for the imperial period, characterizes Daniel Hoyer's *Money, Culture, and Well-Being in Rome's Economic Development, 0–275 CE*.⁵ Hoyer's interest is in the economy of the high (western) Roman Empire, and, like Roselaar, he is influenced by NIE. But his work is distinctive in three ways. First, he contends that traditional NIE too often assumes a clear distinction between 'public' and 'private' activity – that is, between the actions of the state and those of private citizens – which simply did not exist in antiquity. Second, he stresses the importance of evolutionary cultural traits on economic behaviour, specifically those 'that either promote or hinder the proclivity of different groups of people to cooperate – termed *prosociality* when cooperation on a large scale is achieved' (17). Third, he insists that we should replace considerations of GDP with measures that take more seriously 'overall social well-being' (15), in line with most modern economists. These starting points make Hoyer's book a significant and welcome step beyond many current approaches to the subject.

The book has five substantive sections. Chapter 2 uses a database of perpetual endowment inscriptions in the western empire as a lens to study the prosociality of Roman elite competition. Hoyer argues that Roman society's early struggles created a culture of cooperation that became the necessary backdrop for Rome's eventual Mediterranean dominance. The well-known phenomenon of elite benefaction should be seen as one manifestation of that Roman prosociality. The perpetual endowment inscriptions elaborate one key economic aspect of this; they 'are epiphenomenal of a wider, substantial, and sophisticated market for credit and investment capital that existed in the Roman world, one which operated largely out of the sight, or regulation, of the imperial state' (19). Detailed study of how the funds were administered, who established them and who benefited, what sort of capital was used and where it went, point towards the creation of investment capital, credit circulation, financial mediation, and market growth. Rome's cultural evolution, in other words, enabled the development of 'a fairly advanced financial economy' (32). Chapter 3 looks at the opportunities for investment in the Roman Empire, its (minimal) regulation, and Rome's productive economy. It includes a particularly interesting discussion of numismatic material, arguing for 'a highly monetized economy featuring a multi-level minting policy supported by the Roman state for a complex mix of reasons. . .the issuing, circulation, and use of coinage, then, has everything to do with market activity' (71–2). Chapter 4 uses a database of North African building dedication inscriptions to demonstrate (using the same 'epiphenomenal' logic as Chapter 2) that most public building in Roman Africa arose from a combination of public and private agency, since the same individuals making private benefactions were the usual holders of public office.

⁵ *Money, Culture, and Well-Being in Rome's Economic Development, 0–275 CE*. By Daniel Hoyer. *Mnemosyne* Supplement 412. Leiden and Boston, MA, Brill, 2018. Pp. xiii + 215. Hardback €98, ISBN: 978-90-04-35827-0.

Again, prosociality rears its head, and reveals something of the underlying principles on which Rome's economic ecosystem was built:

the grand bargain of the Roman Empire allowed the imperial state to draw enough of the Empire's surplus revenue to support a monopoly over military power as well as reward associates and supporters and to subsidize the population of the metropolis, Rome. In return, local elites were given a great deal of leeway with minimal state oversight to capture a fairly large share of the remaining free-floating resources and to protect their own interests – economic, social, and political – as they saw fit. (101)

This aspect of Hoyer's model will be the least controversial, since it aligns with the consensus among Roman historians concerning Roman governance. Chapter 5 aims to use 'well-being' to shed new light on the classic – and unresolved – question of whether Rome enjoyed intensive or extensive growth (namely, whether the growth of the economy outstripped that of the population, and thus represented *per capita* growth). In reality, much of the chapter consists of a mission statement arguing that such work should be done, rather than the actual work itself. But a concluding discussion of wealth circulation and the creation of public goods points simultaneously towards 'a widening gap between rich and poor' and beneficence that 'allowed many poor Romans to have reasonably high standards of living in spite of the gross disparities in wealth' (135). Chapter 6 then seeks to test Hoyer's overall model by looking at the economics of the alleged 'Third Century Crisis', a period typically neglected by economic historians. Hoyer demonstrates that the empire split into 'distinct monetary zones: the relatively early and severe debasement of the traditional denominations along with the early introduction of the *antoninianus* in northwestern Europe; and the continued use of the traditional denominations at relatively stable weights and values in southern Europe and Africa' (147), a divide that echoes the regionality of military action and disease outbreaks in the period. Volatility in the affected regions impacted not just the elite ethic of benefaction but also the wider market of investment and credit for which Hoyer argued in the preceding chapters (thus providing, in his view, further evidence of their existence). These failings were solved by, among other things, greater centralized control and thus the establishment of firmer divides between private and public – in other words, by a new economic model.

There is much to recommend this book. It is based on Hoyer's 2014 dissertation, a focused study of North Africa, but the revisions have been influenced by his current position at the interdisciplinary and comparative 'SESHAT: Global History Databank'. That professional interdisciplinarity has produced concrete scholarly rewards: economic historians of antiquity are more than usually willing to engage in heuristic comparative and theoretical work, but Hoyer pushes the field even further by beginning from not one but multiple new starting principles. In addition, the close studies of 105 endowment inscriptions and 602 North African public building inscriptions in Chapters 2 and 4 (see the two appendices at 163–8 and 169–89) are innovative, rigorously researched, patiently constructed, and, because of that, persuasive. And the overall, empire-wide model of the Roman economy built upon them seems plausible. But precisely that transition from persuasive to plausible also reveals something of a disconnect between the promise and the actuality of the book. Hoyer's concrete research focuses on very specific material of which he is in total

command. But he wants to offer a cohesive model of the Roman economy as a whole. That is admirable. However, doing so simply requires more in-depth research than is evident in these 161 substantive pages. Hoyer refuses, for example, to engage in any detail with the challenges to his model posed by earlier work: 'I will admit that these points are fairly provocative, as much ink has been spilled by Roman historians over the years arguing over the extent of market integration and monetization in the Roman world, or lack thereof. I do not wish to rehash all of these arguments' (78). So how can the reader (particularly the non-expert outsider Hoyer claims to be targeting) judge the relative merits of his position?

In the absence of the massive amount of work needed to substantiate the model, then, too much of the book, outside its central case studies, must rest upon a near-constant rhetoric of 'must', 'could only', 'undeniable' – language that will provoke suspicion precisely because it is typically only needed as a replacement for substantive argument. True, some of our questions relating to the ancient economy can be answered only with suggestions simply because of the nature of the evidence, but they are not as many as receive that treatment here. Hoyer is, I hasten to add, aware of this – hence the other repeated rhetoric of the book: an insistence that he merely wants to offer a synthesis of the work of others, aimed in part for an audience in other fields (e.g. 'my primary aim here is survey and synthesis', 13). Again, admirable, but then one wonders if Brill's 'History and Archaeology of Classical Antiquity' is the right venue. And that rhetoric actually undersells the scholarly importance of Hoyer's own case studies. Finally, this gap between aspiration and reality forces Hoyer to constantly apologize for omissions – the east, the climate, and so on – which he would not need to excuse if the scope of the book was more appropriate. In short, then, it feels like Hoyer wants this book to be something it is not yet ready or capable of being: a 'theory of everything', but without the everything. But lest this seem too negative, let me repeat that the hypothesized model here offered could yet prove to be an important one.

Some of the heavy lifting required to do that proving comes in the form of a substantial new edited collection, the latest in the fecund Oxford Studies on the Roman Economy series. *Capital, Investment, and Innovation in the Roman World* explores in detail several aspects of the Roman economy that Hoyer thinks central.⁶ Indeed, the editors, Paul Erdkamp, Koenraad Verboven, and Arjan Zuiderhoek, begin their Introduction with an inscription parallel to those Hoyer discusses – a private investment in workshops, by public office holders, intended to generate revenue for an annual benefaction to their fellow citizens (*CIL* 9.2226). This volume also affirms the importance of cultural evolutionary traits for economic history: 'the allocation and exploitation of what we call capital, whether capital goods or financial capital, i.e. credit... was often strongly determined by the specific character of Roman social, political, and cultural norms, practices and institutions' (2). In fact, Hoyer and the editors here take the same late work of the economist Douglas North as their starting point.⁷ Their goals are also similar, since Erdkamp, Verboven, and Zuiderhoek want 'to investigate if the

⁶ *Capital, Investment, and Innovation in the Roman World*. Edited by Paul Erdkamp, Koenraad Verboven, and Arjan Zuiderhoek. Oxford Studies on the Roman Economy. Oxford and New York, Oxford University Press, 2020. Pp. xx + 487. 53 b/w illustrations. Hardback £100, ISBN: 978-0-19-884184-5.

⁷ D. C. North, *Understanding the Process of Economic Change* (Princeton, NJ, 2005).

Roman economy indeed possessed the structures, in terms of factor endowments and the exploitation thereof, that would lead to the modest but sustained *per capita* growth that is often postulated' (4–5). In fifteen papers, divided into three substantive sections ('Investment and Innovation', 'Capital and Investment in the Rural Economy', and 'Human Capital, Financial Capital, and Credit Markets'), contributors offer the kind of diverse, detailed studies needed to approach an answer. As the Introduction (1–38) make clear, in terms of capital, the volume is most interested in privately provided capital, and covers agriculture, shipping, and urban workshops (e.g. Brokaert and Zuiderhoek, 99–146). On investment, it covers underlying attitudes across Roman society (noting that status-orientated and income-orientated mentalities were not mutually exclusive), its objectives – namely maintaining, consolidating (e.g. Andreau, 417–36), and increasing wealth (e.g. Verboven, 381–416) – financial assets and credit markets (e.g. Gregoratti, 461–80), public contracts (e.g. Andreau, 417–36) and public capital goods (e.g. Ronin, 225–52), the institutional frameworks that helped or hindered investment (e.g. Marzano, 275–306), and the contribution that investments made to the wider economy. It looks at innovation in technology, scale/standardization, and implementation, both of product and of process (e.g. Wilson, 147–94), but also at the wider social and economic context surrounding that innovation that meant it did/did not contribute to economic growth (e.g. Lewit, 307–56).

Worthy of mention too is another offering in Brill's Research Perspectives series, Fleur Kemmers' *The Functions and Use of Roman Coinage*.⁸ In this brief bibliographic essay, Kemmers first walks through how Roman numismatics has been transformed over the last few decades (the late twentieth century also makes a cameo appearance), not just in its practice but also in its self-conception (1–5). She then sketches the most recent methodological, theoretical, and – perhaps most importantly – technological developments in the field (6–8). This covers the explosion in the material available for study (due both to the public's increased use of metal detectors and to the institutional schemes that record their findings), increased sophistication in archaeometallurgy, and the emergence of NIE. The next section looks at the classic chronological and geographical division of material into 'Republican', 'Imperial', and 'provincial', with a separate consideration of coin hoards (8–15). Kemmers provides particularly helpful practical details of where materials for particular research questions might be found, including the field's ongoing, and increasingly harmonized, digital efforts. The two substantive chapters focus on what Kemmers sees as coins' two key roles: communicative (15–37) and monetary (37–53), since they are both 'ego documents...[and] objects used in everyday life' (3). On the first, she traces scholarship's complete about-turn from widespread denial of coins' political value, to recognition of their complex and varied ideological roles. On the second – less discussed by ancient historians – the aforementioned technological innovations have enabled new conclusions to be drawn on metrology and fineness, as well as the provenance of bullion. The final chapter looks at coinage usage, in particular Rome's impact on local practice, and the impact of her infrastructure on coin circulation (54–63). These chapters treat

⁸ *The Functions and Use of Roman Coinage. An Overview of 21st Century Scholarship*. By Fleur Kemmers. Brill Research Perspectives. Leiden and Boston, MA, Brill, 2019. Pp. vi + 84. Illustrated. Paperback €70, ISBN: 978-90-04-41352-8.

Republican, Imperial, and provincial material in turn; as Kemmers notes, it is the third where the greatest progress is currently being made. Indeed, to the communicative function of provincial coinage might be added the recent revelation that provincial coinage did not just respond to central initiatives, but in fact influenced central representation in turn.⁹

Kemmers' survey is lucid and does an excellent job in drawing out from the flurry of competing publications the overall themes and directions driving the field. Particularly interesting is Chapter 5's discussion of coinage distribution. The spread of bronze coinage does seem to support state intervention in its distribution, surprising as that may seem when we consider the logistics involved (47–9); this is precisely what Hoyer's third chapter predicts (at e.g. 74). Indeed, Hoyer's work fits well into Kemmers' final clarion call that 'studies of the Roman economy would do well to consider monetary practices as one aspect' (63). I remain a little confused, I admit, as to the scope given to authors in this series, since there is significant variation in how they interpret their brief.¹⁰ In this case, I regretted the exceptionally short timeframe: yes, consideration of the previous century of research would have expanded the work's length... but the volume is only sixty-four substantive pages, and other publications in the series are twice that length (and research before the turn of the century was rather less abundant).

The two-volume edited collection *Roman Law and Economics* provides a partial segue to legal matters.¹¹ These volumes, edited by Giuseppe Dari-Mattiacci and Dennis P. Kehoe, seek to unite those historians who use modern theory to shed light on Rome's law and economy, and those modern lawyers and economists who see antiquity as a testing ground for contemporary theories, in particular the impact of institutions on economic performance – NIE again (i.v, i.2). As with the two previous works reviewed, this volume is interested in the question of economic growth and its distribution (i.v). The editors have invited twenty-four heavy-hitters from both worlds (an extraordinary twenty-seven out of thirty contributors hold chairs) 'to consolidate a new field of research, the economic analysis of Roman law' (i.vi). Their goals are, first, to provide 'a novel perspective on the function, evolution, and, possibly, rationale of Roman legal institutions', and, second, to offer 'a radically interdisciplinary methodological toolbox to the analysis of Roman legal institutions'.

The volumes do, I think, validate the editors' intentions, at least in terms of individual papers. In the first category, we might point to Verhagen's paper treating the law on secured lending transactions (ii.113–58). As for the second, a rather good example is one of Abatino and Dari-Mattiacci's papers (i.273–306), applying agency cost theory to Roman businesses run by slaves. The volume's claims, of course, depend on twin assumptions. The first is that there is more that unites people in antiquity and today

⁹ E.g. O. Hekster, *Emperors and Ancestors. Roman Rulers and the Constraints of Tradition* (Oxford and New York, 2015).

¹⁰ See my comments on two previous volumes in the series in *G&R* 67.1 (2020), 95–6.

¹¹ *Roman Law and Economics. Vol. I. Institutions and Organizations*. Edited by Giuseppe Dari-Mattiacci and Dennis P. Kehoe. Oxford Studies in Roman Society & Law. Oxford and New York, Oxford University Press, 2020. Pp. xix + 345. 6 b/w illustrations. Hardback £85, ISBN: 978-0-19-878720-4; *Roman Law and Economics. Vol. II. Exchange, Ownership, and Disputes*. Edited by Giuseppe Dari-Mattiacci and Dennis P. Kehoe. Oxford Studies in Roman Society & Law. Oxford and New York, Oxford University Press, 2020. Pp. xix + 435. 12 b/w illustrations. Hardback £95, ISBN: 978-0-19-878721-1.

than separates them: specifically, in economic terms, that they ‘tended to behave in ways that rationally enhanced their prospects of achieving security, prestige, prosperity, enjoyment, and opportunities for their children to have an even better life’ (i.4; ii.1–2). The second assumption is that the same is true of state interest and motivation, namely that ‘the essential challenge remains the same: to establish stable institutions that achieve the benefits of cooperation without creating undue potential for oppression, facilitate value-enhancing transactions, provide opportunities for reducing or limiting risk, and allow at least some people the means to live a good and decent life’ (i.4). These are not, historically, uncontroversial assumptions, but it is I think fair to say that the balance of scholarship now favours such optimism.

Taken as a whole, the volumes are perhaps rather light on cohesion or big picture payoff (the editors themselves offer only a three-page preface; the introductory heavy lifting is left to Geoffrey Parsons Miller’s two opening offerings). But the experiment is at least partially successful, and it is as undeniable as it is unsurprising that inviting commentary on the ancient world from scholars in other fields has the potential both to overwhelm and to inspire. Parsons Miller expresses the additional hope that ‘The study of Roman legal institutions can contribute to the analysis of public policy... In some cases, economic analysis of Roman law may enhance our understanding of economics itself’ (i.4). I leave it to those in modern fields better qualified than myself to judge whether that is the case, but I certainly applaud the effort to reach beyond narrow disciplinary boundaries.

Edited collections on Roman law are, it seems, quite the hot ticket, and Edinburgh the hotbed of research. These twin volumes are published in the Oxford Studies in Roman Society & Law series edited by Edinburgh’s Paul du Plessis, and two more new edited collections on law have also emanated from the Scottish capital, both under du Plessis’ aegis in one way or another. *Roman Law before the Twelve Tables* bites off a self-evidently tricky topic, since the study of law is so text-focused, and the earliest stages of Roman history so text-bereft.¹² Hence the interdisciplinary approach of the book’s subtitle: the editors have sought out scholars not just of law and history but of archaeology and anthropology to try to shed new light on the origins of Roman law in the archaic period. Only with such a skill set can we hope to get beyond the problematic, and brief, narrative assessments of Gaius and Pomponius preserved at the start of Justinian’s *Digest*. In particular, where those sources suggest that before the Twelve Tables there was only custom (and thus that the Tables represent the true beginning of Roman law), Bell and du Plessis want to build on recent work that has shown that the Tables represented instead merely a transition, and assess how much we can say about what came before.

This slim volume contains ten substantive chapters, divided into three sections. The first, ‘The Materiality of Roman Law: New Archaeological Discoveries’, seeks to bring to wider attention new archaeological discoveries that allow us to gain genuinely new purchase on the topic. Here we are offered discussions of inscriptions in regional languages pertaining to legal issues (Clackson, 9–23), the archaeology of Gabii and its

¹² *Roman Law before the Twelve Tables. An Interdisciplinary Approach*. Edited by Sinclair W. Bell and Paul J. du Plessis. Edinburgh, Edinburgh University Press, 2020. Pp. viii + 208. Hardback £85, ISBN: 978-1-4744-4396-8.

consequences for our picture of the social world of Latium and thus the development of law (Naglak and Terrenato, 25–40), and the content and religious significance of the new Etruscan Vicchio Stele (Warden and Maggiani, 41–54). The second section, ‘Constructing Early Roman Law: Sources and Methods’, contains five papers focusing on the *leges regiae* and the Twelve Tables: their creation (Amunátegui Perelló, 57–76), Livy’s account (Rocco, 77–110), the links between the latter and the jurisprudential sources (Laurendi, 97–110), urbanization and the authority of the kings in practice (Smith, 111–31), and the reach of archaic law in relation to the *pomerium* (Armstrong, 133–52). The third section, ‘Roman Law in Historiography and Theory’, expands the volume’s scope to the continuing influence of early German scholarship (specifically Bachofen and Niebuhr) on the field of early Roman law (Capogrossi Colognesi, 155–70), and early anthropological scholarship and its significance for the legal nature of *nexum*, a loan contract so punitive that it created debt bondage (Pottage, 171–98).

Taken together, the papers are most successful in revealing the sheer difficulties associated with the task set. They are thus perhaps best seen as prolegomena to a field in which more work is needed (with which hope the editors end their Introduction, 1–5). To take more substantive steps into such a topic, one suspects a rather larger edited collection would have been needed. And a good model for such a venture would be that on which Kimberley Czajkowski and Benedikt Eckhardt (both of Edinburgh), with assistance from Meret Strothmann, have embarked in *Law in the Roman Provinces*, published in the aforementioned Oxford Studies in Roman Society & Law series.¹³

Czajkowski and Eckhardt take their lead from the ‘growing move to reintroduce indigenous agency and not to take a purely Romano-centric perspective’ (1) in our study of the Roman Imperial project over the last century. For them, that revolution offers both an opportunity and a warning. An opportunity, because our growing database of documentary material evidencing how law was actually used on the ground offers the possibility of applying these insights to the legal sphere. This has produced a spate of regional studies documenting the sheer complexity engendered by the overlap and interplay between local and supra-regional legal systems, even after the extension of citizenship to all in 212 CE (4–5). Czajkowski and Eckhardt see one benefit of their collection as collating these detailed local case studies to try to tease out – with due caution – similarities and dissimilarities across the empire. In particular, recent discoveries of documentary material in the west (the Bloomberg tablets in particular) to parallel that in the east allow a challenge to the traditional divide, observed in studies both of Romanization and of Roman law, between scholars’ views of the eastern and western halves of the empire.

The warning, however, comes because law continues to offer – or at least, has the potential to offer – a challenge to the new consensus. Put another way, provincials could mobilize, appropriate, and manipulate law, but only to a certain extent; fundamentally, law at least partially seems to validate the old centralized model of

¹³ *Law in the Roman Provinces*. Edited by Kimberley Czajkowski and Benedikt Eckhardt, in collaboration with Meret Strothmann. Oxford Studies in Roman Society & Law. Oxford and New York, Oxford University Press, 2020. Pp. viii + 526. Hardback £110, ISBN: 978-0-19-884408-2.

Romanization. Czajkowski and Eckhardt thus dedicate significant space to the ways in which law was used as part of the ideology of empire (5–7). Such ‘empire-wide legislative ambitions’ (7), direct and indirect, include Rome’s totalizing discourse of subjugation, the emperor as universal law-giver, and the largely standardized municipal laws. They were made manifest not just in the formal infrastructure of provincial law – the *leges provinciae*, assize tours, judicial authorities, legal experts, and archives – but in phenomena that acted as indirect vehicles – citizenship, or the military, for example (7–10). One real value of the volume is thus that it unites the ‘top-down’ perspective of law’s role in Imperial ideology with the ‘bottom-up’ approach of its use in practice. Achieving this requires a combination of work on Roman institutions and individual provincial agency, since the reality of Roman law in the provinces is to be found not just in that duality, but in the interactions and even gaps between them. It is this that the editors see as the ultimate intellectual payoff of their volume:

the process of interaction that then arose between the tools offered and the provincial reaction means that the ‘Roman legal order’ in the provinces, or even ‘Roman law’, should be understood as a contingent idea, whose exact features vary from region to region...the legal order becomes part of a dialogue between rulers and ruled that was essential to maintaining the empire’s function. (12)

To that end, Czajkowski and Eckhardt have collected twenty-one substantive papers of high quality, evenly distributed in three geographical sections: ‘Egypt and the Near East’, ‘Asia Minor and Greece’, and ‘Africa and the West’. The number of papers, and the wide geographical coverage, lends real weight to the editors’ belief that their volume read as a whole ‘will, we hope, provide a new and wide-ranging view on law in the Roman empire’ (v). Edited collections, like Roman gladiators, currently have a rather ambiguous status in our field: ubiquitous, but often derided for being so, and consistently undervalued in institutional evaluations of academic research. Certainly those edited collections that apparently exist largely for their own sake leave something to be desired. But *Law in the Roman Provinces*, like *Capital, Investment, and Innovation in the Roman World* above, provides proof of the genre’s essential value in collating a large number of highly specific studies (requiring expertise increasingly impossible for any one practitioner) that enable us to look at antiquity through a wider lens.

Finally, to religion. Paula Frederiksen’s new offering, *When Christians Were Jews*, offers a pillar of the field’s distinctive take on the first pillars of the church.¹⁴ The title (one proffered by Frederiksen’s editor, which preceded the actual writing of the book, and which she partially disowns on the last page) suggests a focus on the close affinities between Judaism and Christianity in the formative stages of the latter. And that we certainly do get in the book’s later chapters. But if anything the title is an undersell, since the earlier chapters deliver fresh and distinctive accounts of a wide range of well-trodden topics in early Christian studies. This, then, is that rarest and most welcome of scholarly beasts: a book that delivers more than it promises.

¹⁴ *When Christians Were Jews. The First Generation*. By Paula Fredriksen. New Haven, CT, Yale University Press, 2018. Pp. viii + 261. 2 b/w illustrations. Hardback £20, ISBN: 978-0-300-19051-9.

Frederiksen's fundamental interest is in the transition period when the earliest Christians' apocalyptic confidence faded to a grudging realization that they had rather more time on earth than they had hoped. That was the moment in which the questions that have shaped the Western world – what they should do, who to involve, how to go about it, etc. – loomed large. But to understand that crucial shift Frederiksen goes right back to Jesus himself. Her first chapter begins by arguing that Jesus' mission was rooted in Jerusalem. To make that case she swims confidently through the turbulent waters of biblical source criticism to argue for the priority of John's account of Jesus' movements over the synoptics. A Jerusalem-focused mission helps to explain the otherwise odd decision of the Galilean disciples to settle in a city in which they had no ties and only traumatic experiences. Important here, too, is Jesus and his followers' attitude towards the Jewish Temple: for Frederiksen, exclusively positive, which leads to her second chapter, on the Passion and Jesus' death. Since it was not his famous Temple cleansing that caused his death, Frederiksen argues that the catalyst for Jesus' execution and the apocalypticism of his earliest followers was one and the same: a change in the rhetoric of his prophecy of the coming of the Kingdom of God 'from *soon* to *now*' (68, emphasis in original). Pilate, chivvied by anxious Jewish aristocrats, executed Jesus as a deterrent to the crowds and the unrest that their heralding of Jesus as 'King of the Jews', and general enthusiasm, risked sparking, but his familiarity with Jesus (a Jerusalem regular) meant he had no real concerns about his followers, and thus left them in peace.

It is thus only in Chapter 3 that we turn to the earliest Christians themselves. Frederiksen's focus here is another classic topic: the followers' motivation to continue after their leader's execution. The resurrection appearances and the expectation of an imminent End were, she argues, mutually reinforcing, and prompted Jesus' followers' decision not just to settle in Jerusalem – 'Everything begins, and events unfurl, from Zion, that is, God's "holy mountain"' (92) – but also to continue his mission to prepare Israel for the coming of the kingdom. Like all good Roman cultural phenomena, the latter was sparked by a combination of tradition and innovation: in this case, an intensive immersion in the Jewish scriptures, combined with the novel idea that the Messiah would come a second time. Chapter 4 considers this further, seeking to explain 'the earliest community's crucial transition from agitated vigil to active outreach' (125). True to their figurehead's example, the earliest Christians were not just Jerusalem-based but Temple-orientated, and had initially unproblematic relations with other Jews and their leaders. Frederiksen then explores how some Christians – and here Paul comes properly into focus – started to adjust their teaching in response to the increasing delay in the expected End. As they continued Jesus' mission to Israel further afield in the Diaspora, they discovered something unanticipated in the synagogues there – gentiles, and receptive gentiles to boot. That only provided further grist to the apocalyptic mill, since scripture predicted the turning of the nations to Israel's god. Finally, Chapter 5 suggests that the earliest persecutions that Christians suffered at the hands of Jews were, like those of Paul, fundamentally internal – that is, disciplinary measures meted out to members of Jewish communities, voluntarily accepted. It was Christian conversion of pagans, and the social unpopularity of the latter's subsequent abandonment of civic worship (from which the Jews were exempt), that prompted such punishment from Jewish leaders anxious about local backlash. Turning to the debates in our earliest documents about appropriate behaviours for Jewish and gentile Christians, Frederiksen elegantly demonstrates how they all ultimately stemmed from

the continuing failure of the Parousia. The Christian leadership remained rooted in Jerusalem, with close ties to other Jewish leaders, right up until the destruction of the Temple in 70 CE, when they disappeared from history.

This monograph represents a scholar in her prime. Only a lifetime of immersion in the materials and reflection on their most difficult problems, combined with the capacity and confidence to write with such a light and effortless touch, could produce a work that remains readable as it picks a novel path through such complex and much-masticated material. The summary above does not do proper justice to how easily Frederiksen manages to not just touch upon, but offer new (and more often than not persuasive) solutions to, the oldest of New Testament critics' concerns. That is not to say that I agree with everything here – in particular, Frederiksen repeatedly assumes a model of exclusive Christian identity that I consider implausible¹⁵ – but this remains the most important, exciting, and enjoyable book in the field I have read for some time.

Frederiksen's book was written in Jerusalem (194), and the holy city is throughout an implicit extra character. Another key city in early Christianity, Ephesus, plays a similar role in Katherine Shaner's *Enslaved Leadership in Early Christianity*.¹⁶ Shaner's goal is to illuminate the complexity of the role and interactions of enslaved persons in Graeco-Roman religion that lie behind the apparently smooth waters of the extant literary and material evidence. In this she takes her lead from feminist historians who have argued persuasively over the last half-century that the rhetoric of our ancient sources and the very questions that scholars ask combine to obscure the realities of ancient female experience (xiii–xvi). Our evidence about the religious dealings of the enslaved should not be read as reflecting the realities of their experience, but as attempts to persuade readers of the existence of that reality. This reading strategy has become widespread, but it has not yet been employed effectively to the nexus of slavery and religion. Shaner demonstrates that the picture painted by extant textual and archaeological evidence – that the enslaved were easily identifiable as such, and clearly and consistently subordinate – is one produced by those elites that wanted and needed both things to be true. In fact, the inherent ambiguities of the everyday power dynamics of Roman slavery allows for a much more interesting picture of enslaved persons' involvements in the hierarchies of Roman religion (xix–xxi). It is these complex dynamics in which Shaner, reacting against Christian exceptionalism, seeks to root early Christian groups (xxi–xxiv). To that end, her chapters alternate between the wider Graeco-Roman landscape and the early Christian evidence. And throughout it is Ephesus that provides the urban backdrop.

In her first chapter, Shaner takes us on an evocative walking tour through the topography and archaeology of the city. Picking out the harbour, the agora, and the elite Terrace Houses, she shows how even the apparently neutral material evidence is prescriptive – 'arguments created in stone, plaster, and spatial design' (4) – and obscures the ubiquity and complex power dynamics of everyday urban slavery. A stele commemorating the benefactors of a new fishing customs house, for example, contains names usually associated with the enslaved; the Market Gate is explicitly dedicated by former

¹⁵ É. Rebillard, *Christians and Their Many Identities in Late Antiquity, North Africa, 200–450 CE* (Ithaca, NY, and London, 2012).

¹⁶ *Enslaved Leadership in Early Christianity*. By Katherine A. Shaner. Oxford and New York, Oxford University Press, 2018. Pp. xxviii + 207. Hardback £79, ISBN: 978-0-19-027506-8.

slaves, indicating that the enslaved could be found on both sides of commercial transactions; and a residential graffito may preserve an act of enslaved resistance. Chapter 2 focuses on an inscribed decree of the proconsul of Asia, Paullus Fabius Persicus, which stood in the marketplace and theatre. It announces his intention to combat corruption in the city's famous Artemis Temple. His inscription dictates that the city must stop paying the free to do the work of slaves, and that slaves must not be allowed to buy priesthoods – demonstrating not just the rhetorical attempt to tie corruption to enslaved *infamia*, but sure evidence, for Shaner, that until that point the free and the enslaved had shared both low- and high-status jobs, and that the enslaved had had the resources to buy, and subsequently hold, leadership positions. With this fresh in the reader's mind, Shaner turns in Chapter 3 to Paul's discussions of slavery in Galatians 3.28, 1 Corinthians 12.13, and, most pertinently, Philemon. The comparison with Persicus' inscription leads to a parallel conclusion for the last text in particular: that Philemon's slave Onesimos may have held a Christian leadership position. The crux of that argument turns on the phrase, 'so that he might minister (διακονῆ) with me instead of you during my imprisonment' (Philemon 13), which Shaner argues, based on comparative Pauline usage, means that Onesimos was tending not to Paul's physical person, but to his spiritual needs, as he did for the rest of the community.

Chapter 4 turns to Ephesus' Parthian reliefs, seeking to shift our attention from the four prominent sacrificing emperors to the (possibly) enslaved female attendants in the background. Parallel inscriptions in Ephesus (the so-called 'Sacred Law' and the *prytaneis* and *kouretes* lists) make clear that it was slaves such as these who preserved the expert knowledge needed for sacrifice to take place. Chapter 5 then returns to early Christian material, reading Ignatius' *Letter to Polycarp* and the Pseudo-Pauline 1 Timothy as similarly bolstering the authority of free elite males by reasserting the importance of proper household hierarchies against a presumed backdrop where those traditional hierarchies were compromised by enslaved persons serving as bishops and deacons in early churches.

Like Frederiksen's, this is a fine work of scholarship. In taking seriously the need to root Christianity in its Graeco-Roman setting, it takes its place among the best work being produced in early Christian history. Shaner successfully reveals the oft-neglected importance of the enslaved in Graeco-Roman imperial religion, and introduces fresh nuance, ambiguity, and discomfort into some of the most discussed texts in history. That being said, there are, I think, potential problems of methodology. The consistent reading behind the evidence proposed here is in essence a form of mirror-reading: that is, 'what must have been the case to prompt this material to try to prove something different'? That is an established and effective approach to working with a fragmented archive. But it also has known pitfalls.¹⁷ First, there are different ways of reading against the grain, and Shaner in practice oscillates between them. Take her approach to the following passage: 'deacons [should be] revered, not double-tongued, not devoted to wine, not greedy for gain' (1 Timothy 3:8; at 96). Her point – and very valid it is too – is that the author wants to define Christian leadership positions by their virtues rather than by their abilities (and thus implicitly rule out the enslaved from holding

¹⁷ See e.g. J. M. G. Barclay, 'Mirror-Reading a Polemical Letter: Galatians as a Test Case', *Journal for the Study of the New Testament* 31 (1987), 73–93.

them). But this is a different kind of mirror-reading from that employed in other cases, where the assumption is that the reality was simply the *opposite* of what the author says was true – which, in this case, would lead us to assume that deacons were reviled, two-faced, drunk, and avaricious. Both ways of reading are legitimate, of course, but they are distinct. By what criteria do we employ one rather than the other? Second, reading against the grain is always suggestive rather than definitive. We can never know for certain that something is asserted because it was not yet the case; it is the possibility that is important. But this methodology is at times presented here as a tried and tested route to reveal history's secrets, rather than simply an effective means to destabilize its assumptions. So, for example, 1 Timothy and Ignatius' concerns over widows could have been built upon antiquity's stereotypical fears of female authority, rather than the reality of their influence. Linked to this is the third concern, namely that at times there is a slippage between possibility and fact. Something revealed by Shaner's methodology as an intriguing possibility can turn in a few pages to a probability, and a few chapters later into a fact, as for example when 'the possibility of Onesimos's enslavement [a different Onesimos, a bishop in Ignatius' letter]' (90) later becomes simply 'Onesimos's enslaved status' (91; see too 115). Scholars of early Christianity reading this book may therefore need to turn a little of the rhetorical care it urges back on its own pages. But read it they must.

JAMES CORKE-WEBSTER
King's College London, UK
james.corke-webster@kcl.ac.uk

doi:10.1017/S0017383520000315

This is an Open Access article, distributed under the terms of the Creative Commons Attribution licence (<https://creativecommons.org/licenses/by/4.0/>), which permits unrestricted re-use, distribution, and reproduction in any medium, provided the original work is properly cited.

Art and Archaeology

I am no doubt showing my prejudice, but I didn't expect a book on Greek acroteria to make for such exciting lockdown reading. Because of their position high up on temple buildings, extant sculpted materials tend to be fragmentary – and hence pushed to the literal and metaphorical corners of modern-day museums. Look to scholarly publications, moreover, and there is a tendency towards classificatory catalogues,¹ markedly less in the way of theoretical discussion (whether about architectural and cultic framing, for example, historical aesthetics, or the intersection between 'ornamental' and 'figurative' representational modes).

This archaeological and scholarly backdrop makes Corinna Reinhardt's study of 'acroteria and architecture' all the more remarkable.² The author provides an authoritative overview of archaic and classical Greek acroteria, in stone and clay, between the sixth and fourth centuries BC. Particular emphasis is placed on three case studies: the Athenian Treasury at Delphi, the Temple of the Athenians at Delos, and the Temple of Asklepios at Epidauros (287–326). But this is no ordinary attempt at architectural reconstruction, nor any standard classificatory catalogue. Instead, Reinhardt peppers

¹ Typical is P. Danner, *Griechische Akrotere der archaischen und klassischen Zeit* (Rome, 1989).

² *Akroter und Architektur. Figürliche Skulptur auf Dächern griechischer Bauten vom 6. bis zum 4. Jahrhundert v. Chr.* By Corinna Reinhardt. Image and Context 18. Berlin, de Gruyter, 2018. Pp. x + 598. 249 b/w illustrations. Hardback £118, ISBN: 978-3-11-053880-9.